The Axiomatisation of Physics

by Joseph F. Johnson
Math Dept., Villanova Univ.

Abstract

Analysing Quantum Measurement requires analysing the physics of amplification since amplification of phenomena from one scale to another scale is essential to measurement. There still remains the task of working this into an axiomatic logical structure, what should be the foundational status of the concepts of measurement and probability. We argue that the concept of physical probability is a multi-scale phenomenon and as such, can be explicitly defined in terms of more fundamental physical concepts. Thus Quantum Mechanics can be given a logically unexceptionable axiomatisation. We introduce a new definition of macroscopic observable which implements Bohr's insight that the observables of a measurement apparatus are classical in nature. In particular, we obtain the usual non-abelian observables as limits of abelian, classical, observables. This is the essential step in Hilbert's Sixth Problem.

Introduction

Hilbert's Sixth Problem is the axiomatisation of Physics. In 1900 at the International Congress of Mathematicians in Paris, Hilbert proposed 23 problems the solutions of which, he predicted, would require major advances in mathematical technique and lead to major advances in mathematical knowledge. Some few of these problems have been fulfilled neither of these expectations, but most of them have amply repaid expectations. The first six problems were about foundations, of logic, set theory, geometry, and Physics.

The Sixth Problem has not been considered one of his best. It has generally been considered sort of borderline. This paper will argue that that is because it has been misunderstood and Hilbert's original focus, in its historical context, has been lost sight of. The revolutionary developments in Physics that followed 1900 have only reinforced the prescience of Hilbert's own formulation of the difficulty in the foundations of Physics. In fact the essence of the solution has been published, in various pieces, in the physics literature already, and only needs to be clarified and assembled.

1

From the standpoint of axiomatics, Physics has been in a crisis, or at least a mess, from 1927* on: the problem is called that of Quantum Measurement.

So much so, that ever since then, it has often been questioned whether the axiomatic method is even appropriate for Physics. (That, of course, is why Hilbert suggested that mathematicians should do the work on this problem.) One of Hilbert's other foundational problems in mathematics has indeed turned out contrary to his conjecture: Goedel's famous undecidability theorem showed that Hilbert was too optimistic in the statement of his conjecture. Now if in fact it is the case that the axiomatic method of Euclid, Archimedes, Newton, Hertz, Klein, and Hilbert is inappropriate to Physics, that would be a claim whose definitive establishment could only be accomplished by an axiomatic analysis of the foundations of Physics similar to Goedel's. This must be regarded as an open question, but it will be answered by this paper that no such fear is really justified: Quantum Mechanics does not introduce anything new in this regard in comparison to Classical Mechanics, there is nothing in Quantum Mechanics to stand in the way of an axiomatic foundation to Physics such as Newton and Hertz essayed. Even more precisely, there is no need to change normal 18th century logic or philosophy. It is possible to axiomatise Quantum Mechanics, at least, in the same way Newton and Hertz operated with Classical Mechanics.

We will neglect gravity, and assume that Schroedinger's equation is universally and precisely true, and is exactly linear and unitary. One might wonder whether future de-

---

* As J.S. Bell put it in his last published article[3], 'Surely, after 62 years, we should have an exact formulation of some serious part of quantum mechanics? By "exact" I do not of course mean "exactly true." I mean only that the theory should be fully formulated in mathematical terms, with nothing left to the discretion of the theoretical physicist . . . until workable approximations are needed in applications. By "serious" I mean that some substantial fragment of physics should be covered. Nonrelativistic "particle" quantum mechanics, perhaps with the inclusion of the electromagnetic field and a cut-off interaction, is serious enough. For it covers 'a large part of physics and the whole of chemistry.' I mean, too, by "serious" that "apparatus" should not be separated off from the rest of the world into black boxes, as if it were not made of atoms and ruled by quantum mechanics.'

velopments in Physics will render these considerations irrelevant. Many physicists have, privately, felt that the present axiomatic mess of Quantum Mechanics would just be rendered irrelevant, obsolete, by future developments in Physics. The methods of this paper will retain some interest in any case: they are robust, and so would probably carry over to future modifications, even non-linear ones. Some physicists such as Weinberg, feel that the logical structure of Quantum Mechanics is so rigid, so incapable of slight modifications,* that it will persist in any future theory, even at the Planck scale or involving strings or branes. Be that as it may, this paper will cover 'a serious part of Physics.'

The solution to be offered will be essentially statistical mechanical in nature. J.S. Bell[3] has influentially criticised many earlier proferred solutions of a statistical mechanical nature, we will carefully meet the important points he has raised. Our claim is that the underlying physics of many earlier solutions, such as that of H. S. Green[9], Daneri–Loinger–Prosperi [10], Colemann–Hepp[11], is basically sound, and Bell's criticisms can be met by being axiomatically and logically careful about the development of these same physical concepts. The underlying physics is that of amplification due to the coupling between a microscopic system with a larger assembly in a state of negative temperature. In order to meet Bell's criticisms, we will define the concepts of probability and measurement in terms of fundamental undefined notions such as energy, space, and time. Such explicit definitions are the only ingredients really missing from published solutions. Bell's criticisms were substantially logical, foundational, and axiomatic in nature, so it should not be surprising that meeting them requires axiomatic care, even to indulging the axiomatic temperament.

Other attempts (such as Lucien Hardy's) to axiomatise Quantum Mechanics or Quan-

---

* *Dreams of a Final Theory,* New York, 1992, pp. 87-89, 211.

tum Information Theory are quite antagonistic to Hilbert's point of view.

History

In 1900 when Hilbert proposed this problem, the landscape of Physics was very different from now. For a hundred years or more, physical science had been riven by completely incompatible theores: mechanics was incompatible with electricity, thermodynamics with both, and atomism (the contribution of chemists) and the theory of chemical structure seemed like an alien kind of magic. Vitalism and phlogiston theories were tolerated. Each rival theory was put on its own axiomatic basis, and the difficulty was that of reconciling the competing theories and domains. But steady progress was made towards overcoming these contradictions, due not to advances in mathematical technique so much as physical insights. Lorentz and Poincare were just formulating the precise way in which the symmetries of Maxwell's equations were incompatible with the symmetries of Newton's equations, and although this still left electrodynamics in a state of crisis, it was progress. Maxwell and Boltzmann had also made serious progress on explaining heat and thermodyanmics in terms of statistical properties of the Newtonian mechanics of large assemblages of billiard balls of the same size as atoms. Although this still left Statistical Mechanics in a state of crisis because of the problems with irreversibility and ergodicity, it encouraged Hilbert to pose the problem.

Perhaps the reason Hilbert was still optimistic was because within mathematics itself, similar difficulties of competing domains had been triumphantly overcome. In 1600, only geometry was axiomatic, and number theory and algebra were so amorphous that Fermat suffered from a writer's block about number theory which did not affect him when he

wrote his geometric book. By 1700, the new analysis had evolved with its own new undefined concepts, and without any axiomatic order or organisation, and so the three different fields of Geometry, Algebra, and Analysis had the same sort of incompatibility and domain problems that afflicted Physics later. But the heroic labours of Cauchy, Weierstrass, Grassmann, and Cantor put an end to this gradually. It was called the arithmetisation of Analysis: the vague concepts of analysis were given precise definitions in terms of the concepts of arithmetic, algebra, and so on. Klein contributed by unifying non-Euclidean geometry with Euclidean geometry, thus removing the mysticism and philosophical opposition to non-Euclidean geometry. Hilbert and others continued by a formal axiomatisation of Euclidean geometry (as opposed to Euclid's own, which was informal and seemed to require diagrams). Peano, Cantor, and Frege gave clear and unambiguous definitions of 'function', 'variable', 'number.' Russell showed how all of Analysis needed for mathematical physics could be deduced from a dozen logical axioms and a few logical concepts.

Probability theory still stood aloof from this unification and had no axiomatic basis. (Wittgenstein made a stab at it at the end of the Tractatus.)

By 1925 the landscape of Physics had completely changed. But let us not get ahead of ourselves too much. Let us try to see Hilbert's point of view in its context. Although aware of Poincare's work, he did not focus on the crisis in electrodynamics. He focussed on the logical foundations of Boltzmann's work.

"6. Mathematical treatment of the axioms of physics

"The investigations on the foundations of geometry suggest the problem: To treat in the same manner, by means of axioms, those physical sciences in which mathematics plays an important part; in the first rank are the theory of probabilities and mechanics.

This is amazingly prescient for 1900. Special and General Relativity (to which Hilbert contributed Einstein's equation, a few days in advance of Einstein), soon succeeded in

accomplishing the unification of Classical Mechanics with Electromagnetism, without producing any axiomatic messes. But the controversy over the foundations of Statistical Mechanics is especially an axiomatic problem. The task Maxwell and, later, Boltzmann, set themselves was to deduce thermodynamics from Hamiltonian Mechanics. But in order to do this they introduced new methods which were powerful, but lacked proof, and when used without sufficient mathematical care, led to the following obvious contradiction: 'entropy always increases' is in obvious contradiction to the time-reversal symmetry of Newton's laws of motions and to Poincare recurrence. Worse, they introduced new physical concepts without adequate definition: the concepts of probability and mixed state. (Now if one reformulates this, as has been done, in terms of information, this merely transfers the problem to a synonym: one has introduced the new concept of information without adequate physical definition.)

Sometimes an axiomatic analysis of foundations shows that one needs more primitive concepts and more explicit axioms about them than was realised. For example, in the axiomatic analysis of the concepts of geometry, where one found out that Euclid needed more concepts than he had specified: in order to eliminate Euclid's reliance on diagrams and intuition, Pasch, Hilbert, and others, had to introduce explicit foundational (hence undefined and primitive) concepts such as 'between'. Sometimes one finds out that concepts which everyone assumed had to be primitive are in fact reducible to other concepts. For example, in analysis, where Cauchy and Weierstrass showed that fewer concepts were really needed, since undefined concepts like 'limit' could after all be given definitions in terms of basic arithmetic concepts. And Frege, after all, number did not need to be as-

sumed, it could be defined, too. So, Russell: it was no longer true that geometry was about a proprietary concept of point that could not be defined, or 'order', with arithemtic about its own patented undefined notion of number, rather all these concepts previously thought to be foundational and undefined, could be defined in terms of 'true', 'proposition', etc. So, phlogiston got eliminated by having temperature, heat, defined in terms of motion. But, alas, probability and mixed state got introduced as new undefined concepts. So an axiomatic analysis would investigate whether probability was needed, as a new primitive, or whether it could be defined in terms of the other undefined, primitive concepts. Can the notion of mixed state be eliminated? These questions are typical of the axiomatic temperament.

Hertz successfully displayed the axiomatic temperament when he reduced all laws of Newtonian mechanics to one, his own variational principle, and all primitive, undefined concepts to three. After 1900, Minkowski, Hilbert, and Klein dabbled in Relativity to do the same.

Now let us fast forward to Schroedinger's development of Wave Mechanics (the relativistic equation he did not publish till years later, now usually called the Klein–Gordon–Schroedinger–Fock equation) in 1925. All of Physics (except cosmology and gravity) is now explained by one equation. Immediately, mathematicians and physicists under Hilbert's influence began the task of axiomatising the new mechanics. Weyl, participating in a seminar with Debye and Schroedinger, was the first to publish an axiomatisation. Hilbert, von Neumann, and Nordheim attempted one which is now forgotten. Wigner and von Neumann continued these efforts and published one now regarded as definitive of its type. Dirac was

by no means under Hilbert's influence, but under the influence of Sir James Jeans, Charles Galton Darwin, and Ralph Fowler. He published his own, which is not expressed in the Klein–Hilbert style, but is often regarded as equivalent to the von Neumann–Wigner one.

But the resulting system is not a system of axioms in Hilbert's sense (or in Euclid, Newton, Hertz, Klein, Russell, Bell, etc.) Not because of its use of new concepts such as probability or measurement or observer or uncertainty. Wigner analysed exactly what the new system's problem, from this perspective is, and gave it the name Quantum Duality. This will be explained in detail in the next section of the paper, by way of anticipation, we will just say that the same physical situation can be analysed in two different ways by choosing to use some axioms and ignore others, or vice versa, and with different results. The whole point of formalising a theory into axiomatic form is to eliminate the need for discretion, experience, good taste.*

Can some basic concepts be eliminated by being defined in terms of the others? Is there more than one way to do this? What are the exact logical relationships between the different concepts and axioms? Can some be deduced from others? Can the dual overlap Wigner identified be eliminated? The problem Hilbert first called attention to in 1900 becomes still more central in 1927 than anyone else would have thought.

Now let us rewind to Boltzmann, and his irreversibility, versus Zermelo and Poincare. Which Hilbert was concerned with. In the 20's, this problem was solved by Darwin and Fowler. They put the existing statistical mechanics on a completely satisfactory, logically,

---

* 'There is nothing in the mathematics to tell what is "system" and what is "apparatus", nothing to tell which natural processes have the special status of "measurements". Discretion and good taste, born of experience, allow us to use quantum theory with marvelous success, despite the ambiguity of the concepts named above in quotation marks. But it seems clear that in a serious fundamental formulation such concepts must be excluded.' Bell, Beables for Quantum Field Theory. 1984 Aug 2, CERN-TH. 4035/84

foundation and even succeeded in solving some new problems with their methods. Indeed, even by 1905, Jeans had outlined the common-sense but logically insightful resolution of this particular problem.* Again by way of anticipation, we will just outline the solution, which eliminates the need for the concept of probability. Statistical Mechanics is the study of the statistical properties of the 'normal' state (or trajectory), and Jeans gave a reasonable definition of what he meant by 'normal'. Boltzmann's deduction of irreversibility takes place within a statistical model, which makes certain approximations compared to the exact Hamiltonian model. That deductions from an approximation will be of limited validity does not deserve to be considered a crisis or paradox!

Next, let us pass to Hilbert's concern with the use of probabilities in Boltzmann's deductions. Hilbert wisely pointed to two different aspects of probability. The first is the notion of physical probability, as used by Boltzmann in his very formulations. Parallel to this would be to clarify the mathematical definition of probability as used in the mathematical techniques used by Boltzmann and Maxwell in their deductions. So Hilbert called for ultimately defining, if possible, and axiomatising a new undefined primitive, if necessary, of physical probability in such a way that it could be used as Boltzmann used it. And as a useful preliminary to this, clarifying *within mathematics merely*, the axiomatic status or definition of the mathematical concept of probability.

Borel and Levy resisted this to the end of their days, just as did Poincare resist

---

* Some writers have interpreted this to mean that H will continually decrease, until it reaches a minimum value, and will then retain that value for ever after. A motion of this kind would, however, be dynamically irreversible, and therefore inconsistent with the dynamical equations of motion from which it professes to have been deduced. As will appear later, the truth is that we have at this point reached the limit within which the assumption of molecular chaos lead to accurate results. The motion is, in point of fact, strictly reversible, and the apparent irreversibility is merely an illusion introduced by the imperfections of the statistical method. Jeans, *The Dynamical Theory of Gases*, second edition, London, 1916, p. 38.

Hilbert's logical aims. Wiener, however, in 1922, following Frechet, used measure-theoretic notions of Borel and Lebesgue to rigourously formulate the notions of probabilites used heuristically by Einstein in the theory of Brownian Movement. Then Kolmogoroff systematised this and published the influential axiomatisation of probability now well known. From a Hilbertian point of view, what Kolmogoroff did was exactly what he asked for as a preliminary to the ultimate goal. But it should not be called, from a Hilbertian point of view, an axiomatisation of probability: it is quite the other possibility that Kolmogoroff succeeded in establishing: an explicit definition of mathematical probability in terms of prior, already defined, foundational concepts: those of measure. Hence the explicit definition of (mathematical) probability is accomplished, and Kolmogoroff showed that the entire branch of mathematics known as probability theory was incorporated in the grandly unified structure, going back to those dozen axioms of logic ... and no new undefined, primitive concept or axiom was required. Within the scope and aims of the Hilbert problem, Kolmogoroff showed that the law of large numbers, say, was no different from the theorem 1+1=2 in *Principia Mathematica*. This eliminated the mysticism and philosophy from the mathematical theory of probability, and was an essential step in avoiding seeming paradoxes involving continuous probabilities in geometric settings. The theory of stochastic processes could not have developed in the continuous time setting without this. (But Kolmogoroff well knew that it did not solve the ultimate goal of the axiomatic analysis of the notion of physical probability and did not solve the problem of giving a logically unexceptionable definition of the concept of physical probability. This difficulty will be exposited in detail in a later part of this paper.)

Even after this accomplishment, it could be wondered whether the physical concept of probability was amenable to something similar, or would require new, Bohrian complementarity style axioms or new quantum logic axioms or goodness knows what else, or even the abandonment of the axiomatic method in Physics. The point of this paper is to show that the concept of probability in Physics can be defined and the measurement axioms involving it be derived from more fundamental axioms and concepts, not involving the notions of measurement or probability or information or observation.

We should not postulate a measure. We must derive the measure from physical properties of the system. We should not introduce subjective notions (such as information) in order to derive the measure. We may use the notions introduced by Poincare into 'general dynamics' (such as stability or density or 'almost everywhere'). These notions are mere logical combinations of physical notions and do not introduce anything new.

Perhaps it is appropriate to remark here that philosophical commitments should not be allowed to trump physics and logic. If more than one solution is possible which satisfies the requirements of agreement with experimental results and the requirements of logic, then one could let philosophical predilections, like a bias in favour of positivism, help one choose between possible solutions. But not otherwise. Twentieth century physicists are more unanimous in philosophical commitments than they used to be, and this is not necessarily a good thing. There are a variety of respectable philosophical positions even *vis-a-vis* science, which have existed or are possible, and if there is a solution which satisfies the requirements of experiment, logic, and is consistent with any such respectable philosophy, then rival philosophical commitments should not be allowed much weight. Cur-

rently, monism is widely accepted by scientists, but dualism used to be more current, for example, with Newton, Leibnitz, and Hertz* allowed that one had to still keep an open mind between dualism and monism. If the choice has to be made between monism *plus* verificationism *plus*abandonment of the axiomatic method, on the one hand, and dualism, on the other, then that would be an important fact to be established. The reader is asked to proceed without assuming that logical positivism and verificationism are proved. We adopt either Wittgenstein's definition of 'meaning', *viz.*, the meaning of a statement is the state of affairs which would be the case *if* the statement were true, or any other normal meaning that would be accepted by Newton, Maxwell, Hertz, or the classical epistemology of Classical Physics. The reader is asked to put aside any prejudices against counter-factual conditionals. The experience of physics pedagogy in that we sometimes study two systems as if they were isolated, and then as if they were coupled, suggests that counter-factual conditionals are meaningful. Perhaps the reason outlandish philosophies (such as quantum logic, verificationism, restrictive positivist-style rules as to what is meaningful and what is not, complementarity) have been resorted to has been the assumption that this axiomatic mess cannot be cleaned up. Let us see.

Physicists of the twentieth century already cleaned up the physical mess of physical theory existing in Hilbert's day, and without introducing any new messes. With this axiomatic mess cleaned up along Hilbertian lines, it is possible that Hilbert's Sixth Prob-

---

* In the text we take the natural precaution of expressly limiting the range of our mechanics to inanimate nature; how far its laws extend beyond this we leave as quite an open question. As a matter of fact we cannot assert that the internal processes of life follow the same laws as the motions of inanimate bodies; nor can we assert that they follow different laws. According to appearance and general opinion there seems to be a fundamental difference. And the same feeling which impels us to exclude from the mechanics of the inanimate world as foreign every indication of an intention, of a sensation, of pleasure and pain,—this same feeling makes us unwilling to deprive our image of the animate world of these richer and more varied conceptions. Hertz, *Die Prinzipien der Mechanik*, Leipzig, 1894 p. 45.

lem is essentially solved, provided that future developments in Physics do not introduce essentially new axiomatic messes. But it transpires that it is necessary to abandon monism.

The Analysis of the Axiomatic Difficulty

Wigner[1] wrote several fundamental analyses of the problem of quantum measurement, and the formulation of the problem which we adopt here is due to him, he called it the problem of quantum duality. He was aware of the style of thinking in Hilbert's orbit. The deterministic axioms of Quantum Mechanics, in Weyl's formulation, assume, to begin with, that every system is a closed system. To every system is associated a Hilbert space $\mathcal{H}$, and a Hamiltonian operator H. Every physical state of the system is described by a ray in that Hilbert space, as usual, we fudge the identifications and consider a non-zero wave function ψ instead of the ray. If the state of the system at time $t = 0$ is ψ_0, then its state at time t is given by

$$\psi_t = e^{\frac{-2\pi i t H}{h}} \cdot \psi_0 \in \mathcal{H}.$$

But there are separate, probabilistic axioms for measurement processes. For every observable, there is a self-adjoint operator Q on $\mathcal{H}$. If the system in the state ψ undergoes a measurement process associated to this observable, the only possible results are the eigenvalues $\{\lambda_i\}$ of Q. Assuming that ψ is normalised and that its Fourier decomposition with respect to the normalised eigenvectors of Q is $\psi = \sum_i c_i v_i$, then the probability that the result will be λ_i is $|c_i|^2$. Dirac added the following axiom. If the result is λ_i, then the system is, as a result of the measurement process, in the state given by the wave function v_i even though such a transition or jump (called the reduction of the wave packet) disobeys Schrödinger's equation. (We reserve discussion of this for a projected sequel.)

On the other hand, the measurement apparatus itself must be a quantum system, possessed of a Hilbert space $\mathcal{H}_n$ to describe its physical states (n is the number of particles in the apparatus) and a Hamiltonian H_n to govern its time-evolution if it were in isolation. Then the state space of the combined system of the microscopic, as we will call it, system originally under discussion and which is being measured, and the macroscopic, as we will call it, measuring apparatus, is $\mathcal{H}_n^{com} = \mathcal{H} \otimes \mathcal{H}_n$ and the joint Hamiltonian is the sum of the two Hamiltonians which would have governed each system in isolation plus an interaction term (this is almost tautological)

$$H_n^{com} = H \otimes I_n + I \otimes H_n + H_n^{int}.$$

The problem of quantum duality, Wigner[1], was that we have two rather different mathematical descriptions of the same physical process and it is not at all clear how to compare them. In the former description, we are implicitly treating the measurement apparatus as if it were a classical system which did not obey the superposition principle so that we are sure that the result of the measurement process is always a definite pointer position, as it is called, a macroscopic pointer visibly and definitely pointing to one or another of the various possibilities λ_i. Bohr (as quoted in J. Jauch, E. Wigner and M. Yanase[2]) always insisted that the measurement apparatus had to be classically describable and classical in nature. Heisenberg (cf. J. Bell[3]) always insisted that we have to put a cut somewhere, marking off when we use the first three axioms to analyse things, and when we use the measurement axioms. Von Neumann (as reported in Wigner[1]) showed that as long as we do put the cut somewhere eventually, it makes no difference where we put it. Wigner contributed to this analysis, emphasising that on the classical side of the cut

will always be the observer's consciousness, at least. So the methodology of introducing a cut used to be considered the solution to the problem of quantum duality. But this no longer commands a consensus in light of advances in mesoscopic engineering, detection of quantum mesoscopic chaos, macroscopic superpositions of states, and so on.

Einstein asked the vague but profound question whether or not the probabilities of the latter three axioms did not arise from some underlying deterministic dynamics in an analogous way to the way they did in classical statistical mechanics. He seems to have thought that this would mean either revising Schrödinger's equation or perhaps introducing hidden variables. But the logically sophisticated treatment of classical statistical mechanics due to C. Darwin and R. Fowler[4], and extended by A. Khintchine[5], does not introduce hidden variables. For us, the method by which G. Ford, M. Kac and P. Mazur[6] carry out the Gibbs program for the case of harmonic oscillators with a cyclic nearest neighbour interaction is paradigmatic. (J. Lewis and H. Maassen[7] has extended this.) By focussing Einstein's question on Wigner's formulation of the problem, we can answer it positively, taking Schrödinger's equation as the underlying deterministic dynamics. That is, we derive the probabilistic axioms from the deterministic axioms. We make no other assumptions[8] (Farhi, Goldstone and Gutmann made several tacit *ad hoc* assumptions), except that we have to introduce some sort of dictionary that compares quantum states with macroscopic pointer positions, or else the comparison of the dual descriptions is logically impossible. Basically, we push the cut out to infinity. No real quantum system is exactly a measurement apparatus, but the thermodynamic limit of quantum amplifying apparati becomes a classically describable measurement apparatus which exactly verifies the three

probabilistic axioms in the limit as $n \to \infty$.

Physically, this model has much in common with aspects of previous work of H. Green[9] and A. Daneri, A. Loinger, and G. Prosperi[10]. One important physical difference with Coleman—Hepp[11] is that the notion of macroscopic which we will introduce is the opposite of their notion of local observable (which is from the theory of infinite volume thermodynamic limits, but not physically appropriate here). The idea that coupling a Brownian mote to a negative temperature amplifier will amplify the motion of the mote from quantum motion, where observables do not commute, to classical motion, where the non-commutation becomes negligible, is perhaps due to J. Schwinger[12]. Our model is that of a non-demolition measurement, however, and we assume that the amplifier exerts zero force on the incident particle.

This has mathematical similarities to the literature on the problem which takes the open system approach. But the physics is different. We make the transition to irreversible classical stochastic dynamics a function of the coupling between the apparatus and the microscopic system. W. Zurek[12] and others[14] such as M. Collett, G. Milburn, and D. Walls[15] use a sort of open systems approach in that they put the interaction which is supposed to turn quantum amplitudes into classical probabilities in the coupling with the environment instead of in the coupling with the amplifier. We assume instead a closed joint system. In principle, this difference should be detectable by experiment. We also predict that the degree of validity of the probabilistic axioms should worsen as the size of the amplifying apparatus approaches the mesoscopic or even microscopic. This should be detectable by experiment as well. The only real novelty is the notion of macroscopic which we introduce.

The need for a precise definition of macroscopic has long been felt. Indeed, it is not possible to compare the two dual descriptions of the measurement process unless some sort of dictionary is provided. Von Neumann and Wigner[16] (footnote 203) seems to have missed the need for a relatively sophisticated dictionary, and simply assumed that a macroscopic pointer position corresponded, more or less approximately in the strong topology on $\mathcal{H}_n$, with a large set of wave functions. It seems to be this unquestioned assumption which has been the obstacle to progress along Wigner's original lines. Abandoning it may mean the abandonment of the ancient dream of psycho-physical parallelism. ([16], p. 223.)

In connection with this I quote P. Dirac[17]. "And, I think it might turn out that ultimately Einstein will prove to be right, ... that it is quite likely that at some future time we may get an improved quantum mechanics in which there will be a return to determinism and which will, therefore, justify the Einstein point of view. But such a return to deteminism could only be made at the expense of giving up some other basic idea which we now asume without question. We would have to pay for it in some way which we cannot yet [1977] guess at, if we are to re-introduce determinism." In this regard I wish to point out that there is almost no credible evidence in favour of the hoary psycho-physical parallelism, and there is a good deal of evidence for the time-dependent form of Schrödinger's equation's being absolutely linear and unitary.

It will be noticed that our physical interpretation, then, is that there are only waves. There are no particles. The problem of quantum duality, then, becomes the problem of calculating, relatively explicitly, how it is that the interaction of the wave of, say, a microscopic system with one degree of freedom (called, sentimentally, an incident particle)

17

interacts with the wave of the amplifying apparatus to sometimes (with a definite probability) produce a "particle-event," i.e., the registering of a loud click on the part of tha apparatus, and other times, produce no such detection event. Thus the seeming particle-events, the seeming detection of particles, is an artifact of the amplification process in the interaction of two waves.

Feynman, famously,[18] did not think so, but he did think that perhaps a little more could be said about the problem of wave-particle duality (p. 22):

"We and our measuring instruments are part of nature and so are, in principle, described by an amplitude function satisfying a deterministic equation. Why can we only predict the probability that a given experiment will lead to a definite result? From what does the uncertainty arise? Almost without a doubt it arises from the need to amplify the effects of single atomic events to such a level that they may be readily observed by large systems.

" ... In what way is only the probability of a future event accessible to us, whereas the certainty of a past event can often apparently be asserted? ... Obviously, we are again involved in the consequences of the large size of ouselves and of our measuring equipment. The usual separation of observer and observed which is now needed in analyzing measurements in quantum mechanics should not really be necessary, or at least should be even more thoroughly analyzed. What seems to be needed is the statistical mechanics of amplifying apparatus.

It is usually not necessary to think about the philosophical meaning of probability, as A. Sudbery[19] remarks, "It is not in fact possible to give a full definition of probability in elementary physical terms." "Attempts to define probability more explicitly than this are usually either circular ... or mysterious ... This is not to say that the question of what probability means, or ought to mean, is not interesting and important; but the answer to that question, if there is one, will not affect the properties of probability that are set out here, and we can proceed without examining the concept any further." All that is necessary is to straightforwardly imitate the classical methods of statistical mechanics, in a physically appropriate way. Remarkably, the result we arrive at corresponds exactly to

J. von Plato's philosophically motivated amendment[20] of the traditional frequency theory of probability. The naive frequency theory suffered from grave logical circularities and Professor von Plato fixed these by incorporating a physical, ergodic dynamics. mechanics. His work has not received the attention it deserves because he had to rely on the underlying dynamics' being deterministic, so it was usually assumed that his purely logical, almost antiquarian concerns, could not be relevant to a quantum world. This turns out to be a misapprehension. It is, however, useful to revise Professor von Plato's theory in two ways. Firstly, time averages are not, for us, the definition of probability, they are the definition of measurement. Secondly, we need to extend it to dynamical systems which although not ergodic, are, because of their large number of degrees of freedom, approximately ergodic in some respects, as envisioned by Khintchine.

Analysis of the Role of Probability

Each axiom by itself would be a viable candidate. It is only when all five (let alone all six) are taken together as a system that we get a mess. They do not form an axiom system in the usual sense of the word. This aspect was analysed in the previous section. The aspect to which we now turn is the role and status of the concept of (physical) probability. From now on the word probability by itself always refers to physical probability, and never to mathematical probability. The operationalisation of the concept of probability is going to be retained unchanged as unproblematic: the great practical successes of Physics demand this: we always operationalise the process of measuring a probability as counting the number of successes in trials (repeated trials of an experiment) and dividing by the number of trials. The fact that we always get different answers is not unusual for practical physics, this was true in astronomy as well even in ancient times. But since we do not

19

adopt positivism, this *operationalisation* does not have to be accepted as the *meaning* of the concept of probability.

If we are to formalise the language of Physics, to construct a formal axiomatic system with any such axioms and concepts in it, we must pick which concepts are primitive (undefined), state which axioms they satisfy, and define all other concepts in terms of the given ones. Derive all future physical propositions as theorems, following from the definitions and axioms with no new assumptions. Hilbert would have asked us to investigate whether probability is primitive or not, and, if primitive, what are its axioms, and if not, what is its definition in terms of system, state, and Hamiltonian? Likewise for measurement.

The logical problem of circular definition: an existing consensus

The usual 'definition' of probability is well-known to be logically imprecise, even circular.

'If the experiment is repeated a large number of times it will be found that each particular result will be obtained a definite fraction of the total number of times, so that one can say there is a definite probability of its being obtained any time the experiment is performed.'*

Now, it is not carping or nitpicking to point out that this is, if taken *au pied de la lettre*, untrue. And, if not so taken, it does not count as a definition, from the Hilbertian standpoint. Dirac has not succeeded in defining the concept of physical probability. It is not strictly true because if one large number is a prime, and another large number is a different prime, it is impossible for the same definite fraction of successes to be obtained.

Features which do not, to my mind, count as defects in an *operationalisation*, can be

---

* P. Dirac, *The Principles of Quantum Mechanics*, Oxford 1930, p. 10.

fatal flaws in a logical definition. And that is what we have here.

Many famous mathematicians have shared substantially the same understanding of the logical flaw, circularity, in any attempt to define the concept of physical probability that remains close to the idea of frequency. For example, Burnside,* in a polemic with Fisher, and also Littlewood and Kolmogoroff whom we will quote.

Firstly, Littlewood† explains, in his insular way, the same point we have made in distinguishing between the task of defining the mathematical concept of probability and the physical concept, and also between the tasks of asserting mathematical propositions about mathematical probability, and scientific propositions (or axioms) about physical probability.

> 'Mathematics . . . has no grip on the real world; if probability is to deal with the real world it must contain elements outside mathematics, the $meaning$ of 'probability' must relate to the real world; and there must be one or more 'primitive' propositions about the real world, from which we can then proceed deductively ($i.e.$ mathematically). We will suppose (as we may by lumping several primitive propositions together) that there is just one primitive proposition, the 'probability axiom', and we will call it 'A' for short.

> ' . . . the 'real' probability problem; what are the axiom A and the meaning of 'probability' to be, and how can we justify A? It will be instructive to consider the attempt called the 'frequency theory'. It is natural to believe that if (with the natural reservations) an act like throwing a die is repeated n times the proportion of 6's will, $with\ certainty$, tend to a limit, p say, as $n \to \infty$. (Attempts are made to sublimate the limit into some Pickwickian sense—'limit' in inverted commas. But either you $mean$the ordinary limit, or else you have the problem of explaing how 'limit' behaves, and you are no further. You do not make an illegitimate conception legitimate by putting it into inverted commas.) If we take this proposition as 'A' we can at least settle off-hand the other problem, of the $meaning$ of probability, we can define its measure for the event in question to be the number p. But for the rest this A takes us nowhere. . . . Now an A cannot assert a $certainty$ about a particular number n of throws, such as 'the proportion of 6's will $certainly$ be withing $p \pm \epsilon$ for large enough n (the largeness depending on ϵ)'. It can only say 'the proportion will lie between $p \pm \epsilon$ $with\ at\ least\ such\ and\ such\ probability$ $(depending\ on\ \epsilon\ and\ n_o)\ whenever\ n > n_o$'. The vicious circle is apparent. We have not merely failed to $justify$ a workable A; we have failed even to $state$ one which would work if its truth were granted.

---

* W. Burnside, "On the Idea of Frequency," *Proc. Camb. Phil. Soc.,* **22** (1925), 726.

† Littlewood, *A Mathematician's Miscellany*, London, 1956, p. 32.

Essentially the same criticism was made by Kolmogoroff in his contributed chapter to Alexandroff, Kolmogoroff, and Lavrentieff, ed.s, *Mathematics its content, Methods, and Meaning*, 2nd ed., Moscow, 1956, we cite from a Cold War translation published in 1963 in Cambridge, Mass., p. 239, " . . . it is clear that this procedure will never allow us to be free of the necessity, at the last stage, of referring to probabilities in the primitive imprecise sense of this term."

Three approaches in the literature

Many naive attempts to fix this problem have been published, but none have succeeded. (Professor von Plato's is far from naive and is postponed until later.) We will only deal with von Mises, Hardy, and Farhi–Goldstone–Guttmann.

Richard von Mises carefully analysed the circularity involved in trying to say anything like, the larger the number of trials, the greater will be the likelihood that the ratio of successes to trials will be, for all observers, within a small epsilon of each other, *etc.* This word 'likelihood' is either a synonym, in which case this attempt is circular, or needs a definition, and none has been provided. Furthermore, von Mises opined that no rigourous foundation could be provided for Statistical Mechanics and so one must abandon anything like the frequency theory and take another road. His suggestions for a positive solution need not detain us, even though related ones were proposed by Alonzo Church, later, and in the 60's, Kolmogoroff, since they are remote from Statistical Mechanics, unphysical, and have not been able to justify the use in Physics of probability the way Hilbert demanded. His opinion that a rigourous justification was impossible has been disproved, by recent successes in the theory of Hamiltonian Heat Baths, stemming from the breakthrough work

of Ford–Kac–Mazur[6] in 1965.

Next we will consider one of the Quantum Information style axiomatisations, this one due to Lucien Hardy.

> 'Axiom 1 Probabilities. Relative frequencies (measured by taking the proportion of times a particular outcome is observed) tend to the same value (which we call the probability) for any case where a given measurement is performed on an ensemble of n systems prepared by some given preparation in the limit as n becomes infinite.*

Here, probability is defined (but 'result' and 'measurement' are primitive). The words 'any case' are subtly vague, as we shall see.

Could Hardy's suggestion as to 'the limit of this ratio as the number of experiments increases to infinity' be adopted? Let us concede the meaningfulness of an actual or potential infinity of repeated experiments. The missed opportunity here is, what is the definition of 'limit'? In mathematics, there is no one canonical definition of limit. There are different ones which apply to different situations. There is the limit of a sequence, of values of a function, the limit in mean of a sequence of functions, weak limits, *etc.* (In this paper, we will define a new kind of thermodynamic limit, and argue that it is just what is needed to fix this problem.) None of these apply unless we can argue that the physical situation of repeated experiments is modelled by the particular mathematical concept chosen. (We will, eventually, argue that the axioms allow us to model measurement, but not other repetitious situations, by the Hamiltonian of an amplifier with an extremely large number of degrees of freedom, but a given, fixed, invariable initial condition, which we will analyse the way Jeans suggested).

It is clear from context that Hardy intends the elementary notion of the limit of a

---

*L. Hardy, *Quantum Theory from Five Reasonable Axioms* arxiv.org/quant-ph/0101012.

sequence. So he has defined probability incidentally and by the way, and the major empirical content of his axiom is that the limits always exist. This is surprisingly problematic (no surprise to the experts).

Firstly, mathematical sequences are deterministic: timeless, they have no dynamics. The one-millionth result is already determined in advance. It would be intuitively and emotionally odd if this was the right concept to use for Hardy's purposes. But let that pass. Secondly, and more importantly, not all sequences possess the property he demands, that the fraction does indeed have a limit. This is why there is significant empirical content to his axiom: only those sequences are physically possible which do indeed have a limit.

This simply does not work. If one experimental situation has one sequence, f_n, whose limit is one-half, and a different one has a different sequence, g_n, say, with some limit, too, (for simplicity, we may assume it is one-half), clearly we may physically construct an electronic AND-gate or what-not and make logical, Boolean combinations of the results, and the resulting sequence will be equally physical. But it is easy to construct such sequences whose logical combinations do not possess any limit at all. So we see that the words 'any case' are not really well-defined. It seems pointless to try to introduce a countable hierarchy of patches to this theory by postulating that every reasonably physically implementable combination of sequences must also possess limiting frequencies ... (Perhaps this is the same problem von Mises ran into with his theory of selections from collectivities?) There was never anything physical about Hardy's postulate to begin with.

The above point is an immanent but physical critique of the logical consequences of his empirical assumption. In my opinion it is decisive. An easier (but not decisive) point can be made: the usual laws of probability do not in fact guarantee that such limits exist,

and the propensity theory does not make this assumption. The laws of probability clearly state that the probability that in two rival infinite runs of the same experimental setup, two different limiting ratios will occur, or one limiting ratio will occur in one but no limit in the other, or, no limit in either, is zero, but probability zero does not mean impossible, such a 'violation' of our expectations is physically and logically possible. So Hardy is (and he is allowed to do this, but it seems odd and unwise) explicitly postulating that physics does not obey the laws of mathematical probability, that physical probability, as he has defined it, does not satisfy the axioms of mathematical probability (and this carries over, later, to the laws of non-Kolmogoroffian probability in his other more developed contexts). (His postulate wreaks havoc with the mathematical notion of stochastic independence.)

In brief, the usual interpretation of the laws of mathematical probability as applied to the fair toin coss situation is that it is physically and logically possible for every single toss to come up heads, and that such a 'violation' does not contradict the statement that the probability of a result of heads is one-half. Hardy wishes to make the frequency theory more important than the mathematical laws. To accomplish this axiomatically, he would need to reconstruct the mathematical theory of probability so that it would match his physical postulate.

Can the much-cited paper of Farhi–Goldstone–Gutmann be taken at face value? Its argument rests, crucially, on a careless fallacy of equivocation. They use the same notation for two very different meanings, and at a crucial point in their argument, switch, impermissibly, betweeen the two meanings. I do not doubt for a minute that there could be some physical motivation for supposing that there is a statistical correlation, in all normal

25

applications, between the two different meanings, but they neither make such an assertion explicitly, nor would such an unproved asserion be useful in an axiomatic analysis. Indeed, their explicit claim to have reduced their result to the principle of invariance of physical results under change of co-ordinates suggests that they made the switch inadvertently.

Their key claim is that a formulation of quantum mechanics without explicit reference to probabilities allows them to instead appeal to the general philosophical principle that physical results must be independent of the choice of coordinates used to calculate them, the appeal occurring at a point where a certain map, which they construct, would, they claim, have to be an isometry for this principle to hold good. By construction, that it is an isometry means a probability measure is defined uniquely. This might be fair if they had in fact defined the map and the measure in the very places they claim to have defined them, but they have not.

More precisely, they alert us to their desire "to contruct a transformation from the basis ... of normalised states to the basis ... of infinite norm states." (This is evidently in order to later appeal to the general philosophical principle as stated much previously in their paper, 'the necessity to describe physics in a basis independent manner.') This would be (not a 'transformation', but a) change of basis matrix if it were not for the fact that the two 'bases' are of different cardinalities, one countable, and the other un-countable. What they must mean, from context, is that they desire to exhibit two formulae, (TII) will express the co-ordinates of an element Ψ with respect to the countable basis in terms of any given co-ordinates with respect to the uncountable basis, and (TIII) will be the other way round. That is, the map is the identity map on a quite concrete Hilbert space, $V_c^{(\infty)}$

the element Ψ does not change.

An 'infinite norm state', as usual, is not an element of the Hilbert space $V_c^{(\infty)}$ itself, but is a linear functional defined on a dense subspace. The formulas are, then, only valid on a dense subspace. This, probably, is why they equivocate between whether, for a fixed infinite sequence $\{j_n\}$, their 'infinite norm state' $\langle b; \{j_n\}|$ is a linear functional, or whether it is a mere formal symbol which, when applied to a vector $|\Psi\rangle \in V_c^{(\infty)}$, yields $\langle b; \{j_n\}|\Psi\rangle$, an L^2-equivalence class of functions on the measure space Y of all infinite sequences $\{j_n\}$. In the former case, we are dealing with alternate co-ordinate descriptions of the same vector. (But only on a dense subspace). In the latter case, we are dealing with a concrete map between two different spaces which can be extended to be an isometry. That these two different set-ups can be described by formulas which look alike is irrelevant to general philosophical principles! This is not simply a change of basis.

As we have just elucidated, they switch from interpreting this 'basis independence' as alternate descriptions of the same vector, to interpreting it as a map between two different vector spaces which must be extendable to an isometry on their respective completions. This is fallacious. Our analysis of every naive version of the frequency theory shows that the sticking point is what to do about this negligible, but physically possible, set of 'violators', of measure zero. What they have done is defined a physically meaningful operator F on only a dense subspace of the Hilbert space, then, mapped this by an isometry to a space where they can extend it by a neat formula. But on this other space, it no longer has the same logical and physical significance, when extended. The operator F, when extended, no longer is a 'frequency' operator.

In fact, the paper is just a tangle of conceptual confusions (from the point of view of

axiomatic investigation, or from the standpoint of Hilbert's Sixth Problem).

On page 370 they correctly assert just what we mentioned in our discussion of Lucien Hardy, that the strong law of large numbers says that the set of all sequences whose proportion of heads does not settle down to any limit or settles down to the wrong limit, has measure zero. Then they immediately pass to a non-equivalent statement: $|\psi\rangle^\infty$ is an eigenstate of the operator $F(\theta_i)$. They immediately assert that this last statement is a 'quantum version' of their previous statement. This is just confused. Their axiom **PIV'** clearly states that if a quantum system is described by the state $|\theta_i\rangle$ then a measurement of the observable 'will yield the value θ_i'. They just admitted that there is a non-empty set of sequences for which this is not true. This is Pickwickian, in the Littlewoodian sense, applied to 'will yield'. Whatever can they mean, formally and axiomatically, by 'will yield' if in fact they admit* that it might not yield?

In general, the difference between the strong law of large numbers and the weak law of large numbers is irrelevant for addressing this problem. If the probability is continuous, probability zero does not mean impossible, so no advance has been made over saying the event of a violation of the long-run average's settling down to a definite fraction is 'unlikely'. (Kolmogoroff was certainly an expert in the difference between the strong law and the weak law that Farhi *et al.* make such a big deal of, and his considered opinion on the matter has been recorded.) One could introduce a new primitive physical term, 'negligible', and add a new postulate, probability zero means the event, although possible,

---

* 'For a recent expression of the view that on the contrary there is no real problem, only a 'pseudo-problem', see J. M. Jauch, *Helv. Phys. Acta* **37,** 293 (1964). ... current interest in such questions is small. The typical physicist feels that they have long been answered, and that he will fully understand just how if ever he can spare twenty minutes to think about it.' J. Bell and M. Nauenberg, "The Moral Aspect of Quantum Mechanics," in De Shalit, Feshbach, and de Hove, eds., *Preludes in Theoretical Physics, in Honour of Weisskopf*, Amsterdam, 1966, 279-86

is negligible. Let anyone who wishes to rely on this, be conscientious about writing down all the details. Then the mathematical notion of passing from one function to another one in its L^2-equivalence class could validly be the model of the physical notion of neglecting negligible differences.

Lastly, they still assume observable and measurement as primitives, without any attempt to elucidate the relationship between physical measurement and other physical processes. This means they have proved uniqueness but not existence.

It would be unlikely if the solution to the problem of quantum measurement did not rely on the physics of measurement, but only on axiomatic analysis.

A new approach

The structure of the axioms themselves suggest that there are two scales going on, and that probability is a multi-scale phenomenon. Feynman, as quoted, went into this physically. That will be our approach. Then, to be consistent, one must make measurement a two-scale phenomenon. Further, since observables only crop up in the 'large' axioms, this strongly hints that they are part and parcel of the concepts to be derived, rather than assumed. Our approach will be to use the usual Hamilton-Liouville abelian dynamical variables, and we will not refer to them as observables. We will derive the new properties of (quantum, non-commutative) observables from the definition of measurement and probability. This will become clearer when the model is concretely spelled out. This marks the major logical change between this paper and all previous papers, even recent ones such as Allahverdyan–Balian– Nieuwenhuizen, which assume the usual properties of observables even while they are deducing them from the Hamiltonian and Schroedinger's equation.

Mere logical combinations of concepts are always admissible since they do not rely on new physical concepts. Since the mathematical theory of probability shows that it is equivalent to give the probability measure, or to give the mathematical expectation of every integrable function (since this allows the measure to be re-constructed from these expectations), we need only give a physical definition of the expectations. Then the same logical processes that derive mathematical probabilities from the expectations, can be used to derive the numbers representing the physical probabilities from the numbers representing the physical expectations. And that is how we will proceed.

Einstein's insight, that quantum mechanical probabilities might be a statistical mechanical phenomenon, suggests we look carefully at what Hilbert asked for in the derivation, within Classical Mechanics, of probabilities and irreversible phenomena, from deterministic equations of reversible evolution. To do this we must not postulate ignorance or probability distributions, and must, still, define probability. Now Professor von Plato has done this if the dynamics is ergodic. Also, Darwin–Fowler set up Statistical Mechanics without the use of probability, they used a neutral term 'weight' to make it clear that they were merely making a mathematical definition. In the retrospect afforded by the work of Ford–Kac–Mazur, and looking forward to use in quantum measurement, it is possible to assemble the different contributions of Darwin–Fowler and Professor von Plato along lines Khintchine conjectured, at least in some special cases, to give an answer Hilbert would have accepted as logically 'clean.' We will do this in the next section, and fortunately it applies to at least a toy model of a quantum amplifier, which will follow.

Although logically and axiomatically 'clean', it is convoluted, just as was Russell's

theory of description. But its empirical content is still very close to the logically muddled frequency theory, and this is a serious recommendation. That it is logically convoluted is by no means a piece of evidence against it, since, as Russell remarked in the Introduction to the *Principia*,

> 'The grammatical structure of language is adapted to a wide variety of usages. ... Language can represent complex ideas more simply [than simple abstract ones]. The proposition "a whale is big" represents language at its best, giving terse expression to a complicated fact, while the true analysis of "one is a number" leads, in language, to an intolerable prolixity.'

Russell, according to Wittgenstein, first showed us that the logical structure of a proposition need not be its apparent grammatical structure. And he had the theory of description in mind. The simple word 'the' required an elaborate 'unpacking' to get at its logical function as part of a description. It should not be surprising that if, on physical grounds, the concept of probability should be a two-scale process, then too, its logical structure should be very far from the apparent grammatical structure of the frequency theory or its use in Physics. I hope the reader will excuse any consequent intolerable prolixity.

The Logical Structure of Statistical Mechanics

Our procedure is modelled on that of Ford–Kac–Mazur, which indeed is modelled on the usual understanding of the Gibbs program. (Later we will point out two significant alterations, inspired by work and far-seeing remarks of Wiener and Khintchine.)

In Sir James Jeans, the very sane idea is expressed that Statistical Mechanics is the study of the statistical properties of the normal state* (or trajectory, same thing). The

---

* 'The first object of statistical mechanics is to determine all the "normal" properties of such an assembly and correlate them with the properties of matter in bulk as we know it, when the assembly is in complete equilibrium. ... We may define the "normal" properties of the equilibrium theory in the manner of Jeans, or perhaps more naturally as *all those properties which the assembly possesses on a time*

statistical properties are such as mean, dispersion, correlation, auto-correlation, three-point

correlations, and so on. None of these ideas depend on the concept of probability, they are

'statistical' in the precise sense of being summary statistics meant to convey some idea of

the properties of a large (even infinite) amount of data. The data are conceived of as quite

deterministic. The definition of normal* is not given with full logical rigour, but this will

be fixed later. Jeans's definition of normal† is, a state is normal if the set of other states,

all of whose statistical properties are the same as this given state, is infinitely probable. By

this he means that as the number of degrees of freedom of the system increase, the ratio

of the Liouville measure of this set of states, divided by the measure of all other states

which the system could pass into, goes to infinity. We see at once that no real idea of

physical probability is being relied on here, just Liouville measure (or, rather, the measure

inherited on the surface of constant energy from Liouville measure, or, if more controllable

integrals of the motion are present, the smaller dimensional surface of 'accessible states').

Now every such statistical property involves some kind of time average.

$$\lim_{T \to \infty} \frac{1}{T} \int_0^T f(x_t)dt.$$

The Gibbs procedure was to first argue that these time averages would be equal to phase

averages

$$\int_{\mathcal{M}_E} f(x)d\mu(x)$$

with respect to the microcanonical distribution, and also with respect to the canonical

average.' Fowler, *Statistical Mechanics*, Cambridge, 1929, p. 8.

  * Ehrenfest and Ehrenfest, §18b, *The Conceptual Foundations of the Statistical Approach in Mechanics,* p. 41

  † Jeans, *op. cit.*, Ch. III, §§52,58, and Ch. V, §§87.

(Gibbs) distribution.

$$\int f(p,q)\alpha e^{-\beta E} dpdq \quad \text{where} \quad \beta \text{ is inverse temperature.}$$

The argument was not rigourous. It has been made rigourous in enough special cases that we can have confidence that von Mises was wrong, it will be possible to give the procedures of Statistical Mechanics an axiomatically sound derivation as Hilbert asked for.

Consider a sequence of Hamiltonian (linear, conservative) dynamical systems M_n with degrees of freedom $n \to \infty$. Each system should in some physically intuitive sense be the 'same, except for the increase in degrees of freedom'. The 1965 breakthrough of Ford–Kac–Mazure was to derive Brownian motion (more precisely, the Ornstein–Uhlenbeck process) from a sequence of assemblies of harmonic oscillators with symmetric and cyclic couplings, increasing in strength, and a kind of renormalisation.

The procedure was to consider a dynamical variable f_n on each finite system, M_n (having Hamiltonian, say,

$$H_n = \sum \frac{p_i^2}{2} + \frac{1}{2} Q_n(q_o, \ldots q_n) \quad \text{for} \quad Q_n \text{ a quadratic form .}$$

It was the momentum p_o of the Brownian mote. They then took a kind of phase average of this, its autocorrelation function, $g_n(\tau) = \int f_n(p,q)f_n(p_\tau, q_\tau)\alpha e^{-\beta E} dpdq$. (It has since been shown that their results are insensitive to the choice of the mixed state of the heat bath.) They then passed to the limit, g_∞, as n approaches ∞ using a cut-off procedure.

I want to emphasize the following logical aspects of the procedure. Each M_n is first replaced by a Gaussian, deterministic, stationary, stochastic process (arising from the Gibbs distribution artifially imposed on it). None of these processes are ergodic or Markoffian, so

they cannot be Brownian motion. Next, each process is completely characterised by one

datum, its phase auto-correlation function.* So, each M_n is replaced by this one function

g_n. The individual states are totally gone. Now this sequence of functions has a limit,

uniform on compact sets. (Each g_n is continuous and bounded and quasi-periodic). The

limit function is exponential decay. We have, thus, first passed to a 'dual object' (the g_n),

then taken a limit. Now we un-dualise. There is a unique (up to stochastic equivalence)

stochastic process which is Gaussian, stationary, and has that limit function g_∞ as its

phase auto-correlation function. It is not deterministic, and that is how probabilities arise

from determinism if Statistical Mechanics is done logically carefully. (Furthermore, K.

Hannabuss's use[23] of the Sz. Nagy dilation is in much the same tradition: the dilation is

constructed simply from the correlation functions.)

Notice the following important point. Because we are interested in phenomena in the

limit as n approaches ∞, yet one cannot directly compare a value of f_n on a vector v_n

with $f_{n+1}(v_n)$, we have to study statistical properties instead of individual points such as

$f_n(v_n)$. This is the logical reason for adopting such a procedure.

Because we have adopted such a procedure, we do not need to construct a limit

dynamical system whose individual trajectories are composed of limits of the individual

trajectories of the finite systems. But we adopted such a procedure because in fact it is

logically impossible to do such a construction. This is not a direct sort of limit. The states

of the limit dynamical system are not constructed from limits of trajectories or states of

---

* A standard theorem in stochastic processes says that a Gaussian, stationary stochastic process is determined up to stochastic equivalence by its phase auto-correlation function, and is Markoffian if and only if that function is exponential decay. Further, all higher auto-correlations are easily calculable in terms of the two point ones referred to.

the finite systems. It is a double-duality sort of construction.

Because Ford–Kac–Mazur present this as a 'result' of imposing the Gibbs distribution early, it seems to most undergraduates that the probabilities arise from the imposition of this distribution, and much pedagogical ingenuity has been expended on motivating this particular distribution. But the results are robust and do not much depend on the particular distribution (except that it should not be negative temperature!!!) so this is a red herring.

This does not fit into the Jeans–Darwin–Fowler scheme, but it is routine to adapt these results so that it does. (Although it does not seem to be in the literature anywhere.) As n increases, the measure (on a surface of constant energy) of states which are within ϵ of obeying the law of equipartition of energy and have nearly equally distributed plus and minus signs ('random', intuitively speaking) becomes overwhelming, becomes infinitely probable, in Jeans's terminology (as is well known). Hence such states are the normal state because direct calculation shows their time autocorrelations are all approximately equal and tend, in the limit, to the same exponential decay typical of Markoffian processes. And are all approximately equal to the phase autocorrelations calculated by Ford–Kac–Mazur explicitly. (As always, the phase calculations were much easier than the time averages, and, furthermore, Ford–Kac–Mazur did not have to worry about normal state because the average wiped out the contribution of the 'violators', as we have been calling them.)

This suggests that their postulation of a heat bath probability distribution is without real foundational significance, it is simply a crutch to circumvent a difficult calculation and a difficult argument. But in this linear model, the difficulties are not great.

The limit stochastic dynamical system Ford–Kac–Mazur obtained, the Ornstein–Uhlenbeck process $X(\alpha, t)$ (for $\alpha \in [0, 1]$ with Lebesgue measure) has a normal cell: almost all α are such that the sample path $X(\alpha, t)$ as t varies, has time auto-correlation equal to the phase autocorrelation with respect to all α (which is exponential decay) (since it is ergodic) and so it is a limit dynamical system for the M_n with respect to time auto-correlations, too. As a 'dynamical system' it is a little odd. The 'initial conditions' are the α, which are not points on the 'trajectories' (the sample paths). We have, in fact, left the category of dynamical systems we were working in: the 'limit object', if we insist on having one, is not a stochastic dynamical system in any usual sense, it is merely an abstract stochastic process constructed out of the correlation function. We needn't really have undualised at all! (And, actually, Ford–Kac–Mazur did not trouble themselves to undualise, they simply exhibited the correlation function.) As far as the philosophy of probability is concerned, there seems to be an important lesson in this: probability only arises if we actually depart from the category we were working in, to study an idealised approximation which really belongs to a quite different category.[26,28]

This approach makes it much clearer that probabilities arise only in the limit object, and only because it is a double duality. They did not arise through coarse-graining, subjectivity, ignorance, loss of information, or any extraneous new concept. They are a merely linguistic feature of an unphysical limit which is a good approximation to certain limited, statistical, bulk aspects of $M_{10^{23}}$.

Therefore probability can be defined explicitly in terms of this sequence and the sequence of observables.

Khintchine, in the period from 1943 until his death in 1953, pointed out how true ergodicity ought not to be needed, and how the Darwin–Fowler logical outline (which we, here, attribute to Jeans as well) ought to be workable for a wide class of physical problems, classical as well as quantum. But he was unable to rigorously justify every step, such as approximate ergodicity. The breakthrough model of Ford–Kac–Mazur is what Khintchine was lacking.

But meanwhile, Professor von Plato, of Helsinki University, well versed in issues of logic, foundations, and axiomatics, was from another direction solving the problem of a logically unexceptionable, physically based definition of probability. It assumes the dynamics is ergodic and thus does not make use of the two-scale aspect of Statistical Mechanics, but it can be generalised to apply to approximately ergodic sequences as above.

More importantly, it can be generalised to quantum mechanics, where the dynamics is not even approximately ergodic. The point is that if it is given that a system is in a certain subset of states, 'normal' is a relative term with no probabilistic significance, we could instead simply study the statistical properties of *that subset of states*. If we knew that the system $M_{10^{23}}$ were in a definite state v_o, and if we could somehow justify that its statistical properties could still be approximately calculated by passing to the limit, where stochasticity arises, the essential logical structure of Statistical Mechanics does not change, only its technical calculation techniques may need adjustment. Therefore, the thermodynamic limit of deterministic systems, all of them in a known pure state, could still be a stochastic process. This is exactly what will happen in the case of a quantum amplifier. So for our purposes, the full generality of approximate ergodicity and how to

alter Professor von Plato's theory is not needed. If a phase space consists of one or two points, ergodicity is not hard to investigate. In the negative temperature situation, our limit system will consist of two points. The interesting question of adapting Professor von Plato's theory to the logical structure of classical Statistical Mechanics, as outlined by Wiener and Khintchine, has been the topic for a separate paper.[28]

Nevertheless, we will recapitulate Professor von Plato's major breakthrough in just enough detail so that our adaptation of it in the penultimate section of this paper, to quantum amplifiers, will be seen in context as a modest step further instead of a strange and unmotivated hack.

Professor von Plato's Ergodic Theory of Probability

The Boltzmann-Gibbs development of statistical mechanics, especially as reformulated by Einstein in his doctoral thesis and subsequent work, does not quite follow the Jeans–Darwin–Fowler–Wiener–Khintchine logical structure as we have sketched it. Primarily because it assumes ergodicity, and secondarily because it imposes a probability distribution on the phase space by *fiat* instead of defining probability explicitly. Nevertheless, in a well-known paper[20] Professor von Plato showed, in the Boltzmann–Gibbs–Einstein framework, how to give a logically unexceptionable, non-circular, and physically based definition of probability if the underlying dynamics is deterministic and ergodic. In this section we recapitulate his logical structure.

Fix n and suppose that the dynamical system M_n is ergodic. Suppose that f_n is a fixed dynamical variable on M_n. By Birkhoff's strong ergodic theorem, for almost all initial conditions $x_o \in M_n$, the time average of f_n,

$$\langle f_n(x_o) \rangle_t = \lim_{T \to \infty} \frac{1}{2T} \int_{-T}^{T} f_n(x_t) dt$$

38

exists. By the way, the theorem shows it almost always exists even if the dynamics is not ergodic, as long as the measure $d\mu(x)$ is preserved by the flow $x \mapsto x_t$.

Furthermore, it almost always equals the phase average of f_n,

$$\langle f_n \rangle = \int_{M_n} f_n(x) d\mu(x).$$

Because mathematical probabilities of any event can be reconstructed if all the mathematical expectations of dynamical variables are known (provided, of course, that they satisfy the necessary additivity properties), we will have defined physical probability if we have defined physical correlates to all these expectations. That is, we are reduced to the problem of giving a logically unexceptionable definitiion of the physical expectation of f_n, since f_n was arbitrary. Every measurable subset S of the dynamical system is physical, so we need to give a physical meaning to the expectation of f_n on S. It suffices to assume f_n is the characteristic function of S and give a logical definition to the expectation of f_n on all of M_n. Professor von Plato has provided this in a way that mimics Einstein's use of the concept of probability (expectation) in his work, and in a way that is intuitively close to the idea of frequency.

We define the expectation of f_n to be the time average $\langle f_n \rangle_t$. If the dynamics were discrete and fairly repetitive, as if it were a softball pitching machine, this would *be* the intuitive idea of limiting frequency. But it *is* a statistical notion which does not involve the idea of probability.

More precisely, we define the expectation of f_n to be the value of this time average for normal trajectories, and we know that this exists and is unique by ergodicity.

The break-through improvement over naive frequency theories is that we have a physi-

cal basis: this definition is not logically circular because it assumes there is a deterministic, ergodic dynamics. There is still a negligible set of 'violators' in a physical sense: if we define 'measurement' to mean the physical system is in a definite trajectory with initial condition x_o and measurement of the value of f_n means $\langle f_n(x_o) \rangle_t$, then it would follow that if this point were in the measure zero set such that the limit either were 'wrong' or did not exist, then the actual measurement would not be equal to its expected value, or would be indeterminate. Then the result of the 'experiment' would not verify the 'theory.' This is a *good* feature of Professor von Plato's theory. The difficulty of negligible but real 'violators' ought to be present in any physical theory of probability, his achievment is to make it logically and axiomatically harmless: it is now a feature of praxis, with a theoretical correlative as part and parcel of a well-posed logical structure.

I have argued elsewhere[25,26,28] that three modifications are desirable in Professor von Plato's theory. Firstly, probability (or, equivalently, expectations) should be defined as phase averages and time averages should model measurements. What for him is a definition ought to be re-interpreted as a theorem that probabilities predict the results of measurements but cannot otherwise be directly observed. Secondly, following the Wiener–Khintchine programmatic, we should embed M_n and f_n in a thermodynamic limit sequence, and only assume the usual thermodynamic hypotheses which yield approximate ergodicity for *some* f_n. Thirdly, the underlying deterministic dynamics should be that of Quantum Mechanics in the particular physical setting of a quantum amplifier. Probabilities should not be expected to arise except when microscopic aspects are being amplified to macroscopic dimensions.

Because we are defining probability to be a limit phenomenon, our definition of prob-

ability is an 'unpacking' style definition like Russell's theory of description, rather than a straightforward definition like Professor von Plato's. A statement such as 'the probability that the result of a measurement of Q will yield the result λ is p' is unpacked into a system of statements which are not actually about results or Q, just as the proposition 'The current King of France is bald' is not about a (non-existent) King of France. Our analysis of measurement is that it is an idealised thermodynamic limit. Therefore, our analysis of this proposition is that it asserts that a negative temperature amplifier can be coupled to the microscopic system being 'measured' in such a way that when this combined system is modelled by a thermodynamic limit approximation, the approximation yields a measure on a classical space of results and the measure assigns a measure of p to the labelled result. The physical significance of such statements is that the approximation is an approximation to a time average of a directly physically meaningful function on the actual, unapproximated, quantum phase space of the amplifier. This will become clearer by example. See also the discussion in[28]. Probability is a linguistic feature of an approximation procedure. (If infinite systems *relevantly coupled* to things being measured really exist in Nature, perhaps some of this indirection will be unnecessary.)

The next section gives a toy model, which is basically that of H.S. Green and many others, and the logical status of the three modifications will be clear by example. Because of the conjectured robustness of the limiting procedures of Statistical Mechanics, the future progress of mathematics ought to be able to show that the features of this model hold good in a certain amount of generality. But we use a two-level model as is usual in the discussion of lasers and amplifiers in quantum optics.

The Concrete Model

The picture is of n particles in a line. Each one passes on its state to its rightmost neighbour, and the first one receives its state from the one on the right end (this is the cyclicity). This takes place in time $\frac{1}{n}$. (A more realistic, but still simplified and explicitly solvable, model of an amplifying apparatus is based on the Curie-Weiss phase transition[27]. However, they are not concerned with deriving the properties of observables from the first three axioms, but feel free to use all six axioms indiscriminately. Nor do they care to define macroscopic precisely, but content themselves with showing that the off-diagonal elements in the density matrix decay, in the limit. This does not escape Bell's criticisms* , which we are especially concerned to meet. The underlying physics behind this model and their model is the same as that of H. S. Green[9], *op. cit.*)

From now on, we distinguish between measurement apparatus and amplifying apparatus. The amplifying apparatus we study will be an explicitly given quantum system with n degrees of freedom, M_n, modelled by a Hilbert Space $\mathcal{H}_n$ and with a Hamiltonian H_n. It approximates more and more to a measurement apparatus as $n \to \infty$. The measurement apparatus is a thermodynamic limit of M_n, denoted M_∞, and is a classical dynamical system. Its states are the equilibrium states of the thermodynamic limit, and are not described by wave functions, its state space is a symplectic manifold, not a Hilbert space, has no linear structure, superposition of states is a nonsensical undefined concept for it.

---

* 'The result (15) is [called by Hepp[11]] the "rigourous reduction of the wave packet." If the "local observables" Q (as distinct in particular from the "classical observables") are thought of as those which can in principle actually be observed, then the vanishing of their matrix elements between two states means that coherent superpositions of ψ_+ and ψ_- cannot be distinguished from incoherent mixtures thereof. In quantum measurement theory such elimination of coherence is the philosopher's stone. For with an incoherent mixture specialisation to one of its components can be regarded as a purely mental act, the innocent selection of a particular subensemble, from some total statistical ensemble, for particular further study.' Bell, [3]. This is the criticism of statistical mechanical proposed solutions which is an independent, and prior, issue to any discussion of von Neumann's so-called projection postulate.

Classical mixtures of its states are possible, as always in classical Statistical Mechanics. One can take the viewpoint that measurement apparatuses and processes are unphysical idealisations of the only processes that are physical, the amplifying processes. This is a valid logical interpretation of the measurement axioms (even, after some contortions, the reduction of the wave packet) and it does not involve any change in the operationalisation of the concept of measurement. In fact, it grounds in concrete calculations what used to be operationalised anyway without justification: the fact that an amplifying apparatus must be large before the measurement axioms are verified. A one-atom device does not perform a measurement ... or reduce the wave packet. But if infinite systems *do* exist in Nature, this concrete calculation should yield accurate results. Because it makes solving the problem harder, not easier, we will assume that only finite systems are realisable. But the line of argument we introduce here is extensible and portable. (If new systems, Hamiltonians, even forces of Nature are discovered that still fit into the logical framework of Quantum Mechanics, similar procedures to what we introduce here should carry over and give similar results.)

Let the state space of an incident particle be $\mathbf{C}^2$. This space has basis $\{\psi_0, \psi_1\}$. For each n, $\mathcal{H}_n$ is the Hilbert space of wave functions describing the state space of an n-oscillator system which is an amplifying device. We let $\mathcal{H}_n = \overbrace{\mathbf{C}^2 \otimes \mathbf{C}^2 \ldots \mathbf{C}^2}^{(n)}$.

In the presence of an incident particle in the state ψ_1, the amplifying apparatus will evolve in time under the influence of A_n (called "Ming," since it leads to a bright and clear phenomenon), a cyclic nearest-neighbour interaction which is meant to model the idea of stimulated emission or a domino effect. In the absence of a detectable particle, the dynamics on the amplifying device will be trivial. This means that

$$\mathcal{H}_n^{com} = (\mathbf{C} \cdot \psi_1 \otimes \mathcal{H}_n) \bigoplus (\mathbf{C} \cdot \psi_o \otimes \mathcal{H}_n) \quad \text{and we put} \quad H_n^{com} = I_2 \otimes A_n + I_2 \otimes I_{2^n}.$$ The

intuition is that ψ_1 means the particle is in the state which the apparatus is designed to

detect. but ψ_o means the particle is in a state which the apparatus is designed to ignore.

It will simplify things if we assume n is prime. (The general case can be reduced to

this by perturbation.) Let $q = \frac{2^n - 2}{n}$ (which is an integer by Fermat's little theorem).

Let i be any integer between 0 and $2^n - 1$. There are n binary digits d_i with $i = \sum_0^{n-1} 2^i d_i$ and uniquely so. If $\{|1\rangle, |0\rangle\}$ is a basis for $\mathcal{H}_1$, then $|i\rangle_n = \otimes_0^{n-1} |d_i\rangle$ form a

basis of $\mathcal{H}_n$. The intuition is that the ith oscillator is in an excited state $|1\rangle$ if $d_i = 1$ and

is in the ground state $|0\rangle$ if $d_i = 0$. We also write $|i\rangle_n = |d_0 d_1 \ldots d_{n-1}\rangle$.

We wish to find Ming such that in one unit of time the d_i are cycled as follows:

$$e^{\frac{2\pi}{\hbar} A_n} |i\rangle_n = |d_{n-1} d_0 d_1 \ldots d_{n-2}\rangle.$$

Choose a set of representatives b_i such that every integer k from 1 to $2^n - 1$ can be

written uniquely as $b_i 2^m \mod (2^n - 1)\mathbf{Z}$ for some $1 \le i \le q$ and $0 \le m \le n - 1$, that is,

$k = b_i 2^m + j(2^n - 1)$ for some j but i is unique. (Since $2^n - 1$ and 2^m are relatively prime,

no matter how k and m are fixed, there exist unique b_i and j satisfying this.)

Then each $|b_i\rangle$ represents an orbit under the action of $e^{-2\pi A_n}$. Re-order the basis as

follows: let $v_o = |b_1\rangle$, $v_2 = |b_1 2\rangle$, $v_2 = |b_1 2^2\rangle$, $\ldots$ $v_{n-1} = |b_1 2^{n-1}\rangle$, $v_n = |b_2\rangle$, $v_{n+1} = |b_2 2\rangle$,

$\ldots$, $v_{2n-1} = |b_2 2^{n-1}\rangle$, $v_{2n} = |b_3\rangle$, etc., up to $v_{(q-1)n} = |b_q\rangle$, $v_{(q-1)n+1} = |b_q 2\rangle \ldots$,

$v_{(q-1)n+n-1} = |b_q 2^{n-1}\rangle$, but $(q-1)n + n - 1 = 2^n - 3$, so we have $2^n - 2$ basis vectors

accounted for. Let $v_{2^n - 2} = |0\rangle$ and $v_{2^n - 1} = |2^n - 1\rangle$.

Let V_1 be the space spanned by $\{v_o, \ldots, v_{n-1}\}$, let V_2 be the space spanned by

$\{v_n, \ldots, v_{2n-1}\}$, etc., up to V_q. Let V_o be the space spanned by $\{v_{2^n - 2}, v_{2^n - 1}\}$. The

Ming Hamiltonian operator A_n on $\mathcal{H}_n$ is a direct sum of its restrictions to the V_i. Its

restriction to V_0 is to be the zero operator. Each V_i is isomorphic to V_1 and we give the

matrix of each restriction of A_n with respect to the given bases.

$$
\text{Solving} \qquad A_n = \frac{h}{2\pi} \log \begin{pmatrix} 0 & 0 & \cdots & 0 & 1 \\ 1 & 0 & \cdots & 0 & 0 \\ 0 & 1 & \ddots & \vdots & \vdots \\ \vdots & \ddots & \ddots & 0 & \vdots \\ 0 & \cdots & 0 & 1 & 0 \end{pmatrix},
$$

we obtain a cyclic skew-hermitian matrix, whose i,j^{th} entry, $\frac{-ih}{n^2} \sum_{k=0}^{n-1} k e^{\frac{2\pi i}{n} k(i-j)}$, is approximately (if n is large compared to $i - j$) $ih\frac{(i-j)^{-1}}{2\pi}$ unless $i = j$ in which case $\frac{ih}{2}$.

As usual in classical statistical mechanics, the observables are all abelian, and are given by measurable functions on the phase space, hence f_n is an observable if $f_n : \mathcal{H}_n^{com} \to \mathbf{R}$ is measurable and $f(c\psi) = f(\psi)$ for $c \in \mathbf{C}^\times$. In order to avoid confusion with the orthodox primitive concept of observable, modelled by a linear operator, we will not refer to f_n as an observable and will not use the term 'observable' in our system at all. These measurable functions are dynamical variables, as usual in Hamiltonian dynamics.

The intuitive picture is that this device is getting more and more classical as n goes to infinity. So the energy levels get closer and closer, approaching a continuum, the oscillators get closer and closer which is why the interaction, at a constant speed, travels from a oscillator to its neighbour in less and less time. If we adjusted by rescaling the dynamics to accomplish this, the entries of H_n would diverge with n. We rescale h instead, so that it decreases as $\frac{1}{n}$, This is typical of rescaling procedures in classical statistical mechanics. It is physically meaningful because the thermodynamic limit is never physically real, it is only one of the $\mathcal{H}_n$ which is physically real: n is not a physical variable, it is a parameter. Passing to the limit is only a mathematical convenience to obtain simple approximations for the physical truth about $\mathcal{H}_{1.1 \times 10^{36}}$. Since h is truly small, this yields valid approximations.

We must couple the amplifier to the incident particle. The Hilbert space of the

combined system is $\mathcal{H}_n^{com} = \mathbf{C}^2 \otimes \mathcal{H}_n = <\psi_0> \otimes \mathcal{H}_n \oplus <\psi_1> \otimes \mathcal{H}_n$. So we need only define H_n^{com}, the Hamiltonian of the joint system, by giving it on the first factor, where it is trivial, and on the second factor, where it is H_n. This is the explicit toy model of a quantum amplifier, we have now to study the question, what quantities at each finite stage correspond, in the limit, to a macroscopic pointer position of a classical measurement apparatus?

We need a precise notion of what is a macroscopic system, in terms of the axioms of quantum mechanics, and what is a macroscopic observable. Although our model owes a great deal to the Coleman–Hepp model, this notion is the exact opposite of Hepp's notion of a local observable (which is also the usual one in the infinite volume thermodynamic limit of Haag, Ruelle, and others[21]). We wish to implement the intuition of a function on the phase space which cannot distinguish between states which differ from each other in a finite or negligible number of spots.

In the classical methods of thermodynamics, one worked essentially with one observable at a time and there was really a sequence of them for each n. I.e., for each n, one has f_n a physically significant phase function on the space $\mathcal{H}_n^{com}$, or its classical analogue. And the physical significance is the same as n varies. It could be total energy of a part of the system, for example the Brownian mote. For us, it could be a formalisation of some intuitive idea such as, the percentage of excited particles in the left half of the device. These methods are acutely, if disparagingly, described by R. Minlos[21]. "For a long time the thermodynamic limit was understood and used too formally: the mean values of some local variables and some relations between them used to be calculated in a finite ensemble

and then, in the formulas obtained, the limit passage was carried out." Excellent agreement with experimental results were obtained that way. At any rate, we formally define such a sequence of f_n to be macroscopic if whenever the sequence of norm one vectors $v_i \in \mathbf{C}^2$, $i > 0$, satisfies

$$\lim_{n \to \infty} f_{n+n_o}(v_o \otimes v_{n_o+1} \otimes v_{n_o+2} \otimes \cdots \otimes v_{n_o+n})$$

exists for some n_o and some $v_o \in \mathcal{H}_{n_o}^{com}$, then it is independent of the choice of n_o and v_o.

We now define the family f_n, which in the limit, becomes the pointer position of the measuring apparatus. There is a basis of $\mathcal{H}_n^{com}$ consisting of separable vectors of the form $\psi_{\pi_0} \otimes |\pi_1 \pi_2 \dots \pi_n\rangle$ where, as before, the π_i are 0 or 1. Let C be the set of basis vectors such that all but a negligible number of the π_i for $i < n/2$ are 1 and all but a negligible number of the others are 0. (This is the device being 'cocked' and ready to detect. It is very far from being a stable state, in the limit.) By negligible, we mean that as a proportion of n, it goes to zero as n increases. For v_n any state of the combined system, and we may take v_n normalised of length unity, let c_i be the Fourier coefficients of v_n with respect to the cocked basis vectors, i.e., those in C. Define $f_n(v_n) = 1 - \sum_i |c_i|^2$.

Why f_n?

We now discuss the physical basis for this choice. Why a sum of amplitudes, squared, of the Fourier coefficients? (Up to an additive constant.) Opinions are divided as to the physical reality of the wave function. Certainly it is not easy to directly measure the wave function or quantum state of a system, even such a simple system as a harmonic oscillator. What are reasonable to measure are the amplitudes $|c_1|^2$ of the state. In fact these are the second main object of laboratory measurements (the first is the eigenvalues of H, of course). It is not directly relevant that the interpretation of what one is doing when one

experimentally determines these amplitudes is thus and such. It is done every day with great experimental success. The procedure is a little indirect, but not unduly so. If we can reliably prepare isomorphic systems repeatedly in the same state,—now the current experiments on teleportation do this as a matter of course—, then repeated measurements of trials or measurements of observables give us the relative frequencies we need to estimate these amplitudes.

It has been often argued by many that this is too indirect, especially because any finite process would only achieve the determination of a finite number of these amplitudes. And that therefore the wave function is not real, it is a probability wave, not a real physical state or a real physical object. But this line of argument is philosophical, not physical. The philosophy of positivism does indeed maintain the thesis that the meaning of a concept is *how* one would observe it. And Heisenberg and Mach did indeed maintain that theoretical physics should not use theoretical concepts that were meaningless, in this precise sense. But Feynman, to take only one example,* has argued against this extreme form of positivism.† Only disagreement with experiment, or logical incoherence, should tell against a physical theory, not philosophical strictures such as the positivist's. (Even these are not always decisive: sometimes the logical incoherence has been fixed by theoreticians, and sometimes the disagreement with experiment has been fixed by better designed experiments ...)

Since the squares can be measured, so can their sums, so this is a very natural function

---

* Another is Weinberg, *op. cit.*, pp. 178ff.

† "Another thing that people have emphasized since quantum mechanics was developed is the idea that we should not speak about those things that we cannot measure. ... Unless a thing can be defined by measurement, it has no place in a theory. ... The idea that this is what was the matter with classical theory *is a false position.* ... Just because we cannot *measure* position and momentum precisely does not *a priori* mean that we *cannot* talk about them. It only means that we *need* not talk about them. ... It is always good to know which ideas cannot be checked directly, but it is not necessary to remove them all. It is not true that we can pursue science completely by using only those concepts which are directly subject to experiment." Feynman, Leighton, and Sands, *Lectures on Physics*, vol. 3, Reading, Mass., 1965, pp. 2-8f.

on the quantum phase space to study. It is the very first abelian dynamical observable which would occur to anybody, and in fact it is the trace of a hermitian projection operator, so in a different guise it has been much studied.

Passing to the Thermodynamic Limit: the Ming Effect

Our procedure is modelled on that of Ford–Kac–Mazur, which indeed is modelled on the usual understanding of the Gibbs program. However, a major physical difference is the renormalisation which we introduce. Another is that the physical quantity whose limit they study is an autocorrelation; ours is a macroscopic pointer position. Now we are interested in phenomena in the limit as n approaches ∞, yet one cannot directly compare a value of f_n on a vector v_n with $f_{n+1}(v_n)$. In keeping with the procedures of classical statistical mechanics, one compares time or phase averages of the various f_n as n varies. (Phase averages would be taken over the submanifold of accessible states, it is more convenient for us to deal with time averages. Time averages have been made the basis for Professor von Plato's theory of the meaning of probability statements. Pauli[22] attributes the same idea to Einstein) Let the incident particle be in the state described by any (normalised) wave function in $\mathbf{C}^2$. Let it be $v_0 = a_0\psi_0 + a_1\psi_1$. The amplifier is in the state $|111\ldots000\rangle$ in C. We now calculate the limit, as n approaches ∞, of $< f_n >$, where $< f_n >$ means the time average of f_n taken over a typical trajectory in the manifold of accessible states inside of $\mathcal{H}_n^{com}$.

We will then find a classical dynamical system Ω_∞ which has a mixed state X, which depends on v_0, and a classical dynamical variable F whose expectation values match these limits. At any rate, it is elementary to calculate $\lim_{n\to\infty} < f_n >$, it is $|a_1|^2$. (The microscopic indident particle triggers a domino effect or macroscopic 'flash' which is 'bright

and clear' i.e., 'Ming'.)

We search for Ω_∞, F, and X_{v_0} as above, satisfying

$$\int_{\Omega_\infty} F dX_{v_0} = \lim_{n \to \infty} <f_n> .$$

Let the state of the (classical limit) measurement apparatus where the pointer position points to cocked (and hence, an absence of detection) be the point P_0. Let the state where the excited states of the apparatus are proceeding from out of its initial cocked state, and travelling steadily towards the right, be P_1. Then $\Omega_\infty = \{P_0, P_1\}$. The dynamical variables on this space are generated by the characteristic functions of the two points, χ_{P_0}, χ_{P_1}. Let F be χ_{P_1}. It is the pointer position which registers detection. The mixed state of $\Omega_\infty = \{P_0, P_1\}$ which gives the right answer when the incident particle is in state v_0 is the probability distribution which gives P_0 the weight $|a_0|^2$, and P_1 the weight $|a_1|^2$. This is precisely what it means to say the the measuring apparatus will register the presence of the particle with probability $|a_1|^2$, and its absence with probability $|a_0|^2$.

Discussion of macroscopic

As Bell often remarked, there is no place in the physical world where one can put a precise cut between the microscopic and the macroscopic, and orthodox approaches to the foundations of Quantum Mechanics fail to be a logical axiomatisation for precisely this reason. He never remarked that there was a difficulty in giving a precise definition of probability. It has transpired that these two difficulties are connected. We have made 'macroscopic' a linguistic notion so that statements using it are seen to have reference to an approximation scheme rather than directly to elements of physical reality. (The cut has been pushed out to infinity, *i.e.*, there is no real cut). The sticking point in all attempts to

define probability which remain close to the frequency theory has always been what to say about the negligible set of violators. In our re-deployment, the notion of macroscopic is that a phenomenon, such as pointer position, is macroscopic if it is defined after neglecting a negligible set of position violators. For example, we defined a variable on the quantum phase space which was insensitive to 'missing' parts of the needle pointer if their bulk was too small to be seen by the naked eye. It's limit was, then, one that neglected it. Because of the situation of amplification, there are two physical scales, so it is physically natural to introduce this kind of concept of negligibility. So, what was a 'sticking point' in other schemes, is now quite natural. This answers Feynman's question as to why probabilities arise from the necessity of amplification. If the naked eye did not neglect such things, there would be no need of amplification. Since there is such a need, the naked eye does neglect them. Neglecting 'violators' seems to be the characteristic feature of frequency-base definitions of probability, and we have the same feature here too, only in a logically 'clean' location.

Discussion of Paralipomena

'Event' is not a concept that can be expressed within the fully formalised, adequately axiomatised language of Physics. It has been left out of our short list of fundamental concepts and it is not defined. (The list was: system, state, Hamiltonian, time.)* Physically, there is something 'noisy' about actual, finite size, event counters. This suggests that 'event' should not be made one of the fundamental primitive concepts, and it even suggests it should not even be considered as exactly physical, but only an idealised approximation such as occurs in the thermodynamic limit. 'Event' seems to be a classical

---

* Compare this to Hertz's list, which was space, mass, time.

concept of limited validity, no wonder the probabilities arise when we start talking about 'events'. The word does not occur in the first three axioms of Quantum Mechanics, so we did not exert ourselves to introduce it. It was a mistake for Born to try to interpret the wave function in terms of events. What we have done instead is to derive the Born rule as a useful approximation in certain physical situations. This is not an interpretation.

It is true that individual consciousnesses certainly experience 'events' and, indeed, experience them singly and one at a time. What we have succeeded in defining in terms of the fundamental axiomatic concepts of Physics is only some statistical properties of (suitably appropriate) sets of events. Nature and Heisenberg have taught us that it is only these statistical properties which are replicable. Now, a 'result' is not an 'experimental result' unless it is replicable.* Therefore the requirement of agreement with experiment is satisfied by our theory, because all replicable statistical regularities of the experiences of events by individual consciousnesses can be stated precisely in the language of Physics, calculated, and the results do in fact agree (to within experimental accuracy which, *inter alia* means neglecting negligible events as a matter of *praxis*) with experiment.

In fact the following statement is also capable of precise formulation and is verified: each single event results in a definite result. Note well that this statement is itself a statistical regularity! It is modelled by the feature of our formalism which says the thermodynamic limit system has precisely two points. The only possible events are the points. Not superpositions. It is a classical system.

No statements in the precise language of Physics are falsified by experimental results or

---

* Philosophical proof by laughter: every scientist immediately starts laughing when they hear the title of a satirical journal occasionally put out by scientists in America. The title is, *The Journal of Irreproducible Results.*

by experience because, for example, the following colloquial statement cannot be translated into our system: (I am afraid this is very Bohrian but after all, Bohr was not actually very wrong) 'I ran the experiment and found the spin was up.' Within the axiomatised language of Physics, the concept of 'experiment' is merely a statistical concept and only describes an ensemble. No actual dynamical system and initial condition is 'an experiment'. It is an amplifier. The concept of 'measurement' has been defined as a limit concept. Fortunately, the colloquial statement is also not replicable! Therefore, as Heisenberg taught us, it is not *necessary* for a physical theory to account for it.

But, 'I ran the experiment and found a definite result' (or, what is the same thing, 'I ran the experiment once and found that the spin was either up or down') is replicable. It is modelled by saying that the correlation of the stochastic process on our limit (two-point) space *with itself* is unity.

There is a consensus that Quantum Mechanics is mysterious and un-understandable. It might, therefore, be a mistake to propose a theory which eliminates all mysteries. The point of the Hilbert problem is simply to shove the mystery into some *other* location than the axiomatics and logical structure. This paper has shown that the mystery can be transferred into the usual philosophical dualism between Physics (materialism) and subjectivity (Geist). More particularly, there is no physical result which is actually violated by our experience. Every contingency which can actually be stated clearly and precisely in our axiom system is 'subject to the laws of Physics'. This is a true dualism without being either a monotonous parallelism or a total disconnect. But it is not a domain problem: every physically formulatable contingency is within the domain of the laws of Physics (except perhaps negligible contingencies?) It is not as though we can experience

something which 'violates' the laws of Physics.

The disconnect is not total because in our experience we experience subjective regularities of certain sorts, and the language of Physics is capable of describing statistical regularities of the same sort, and the two languages do truly describe, in a concordant fashion, the same regularities. So we have a dualism which is not a parallelism, but has a precise dictionary for some (but not all) concepts. Perhaps other connections remain to be discovered. This is the first such connection which is not a full parallelism to be precisely formulated, but by no means rules out more.

We abandon '*das sog. Prinzip vom psycho-physikalischen Parallelismus*'* in favour of more modest functors between the two sides of the duality.

Summary

This answers a famous question of Einstein's partly affirmatively and partly negatively. It is possible to derive the probabilities in measurement results from an underlying deterministic dynamics analogously to the way it was done in classical statistical mechanics of Einstein's day. It is not necessary to assume that quantum mechanics is incomplete in order to do this: we may take the wave function as the complete description of nature and Schrödinger's equation as exactly and universally valid.

This analysis shows that wave-packet reduction in the strong topology, even approximately, need not occur. It also shows that the transmutation of quantum amplitudes into classical probabilities depends only on the macroscopic nature of the pointer position and its coupling to the microscopic system being measured. It does not depend on any back-force being exerted on the microscopic system. The coupling can be as gentle, in

---

* von Neumann, *op. cit.*, p. 223.

its effect on the microscopic system, as desired. It suggests that the degree of validity of 'measurement,' as an approximation to a physical amplification process, depends on the size of the apparatus. Mesoscopic amplifiers should, then, demonstrate detectable noise phenomena in comparison to macroscopic amplifiers.

This analysis further shows that it is not necessary to invoke the effect of the environment in order to construct a logically coherent theory of decoherence. The fact that probabilities arise even from an amplifier which is in a pure state shows that quantum measurement can be explained without super-selection rules. Thus the question whether the observed behaviour of measurement processes is due to the coupling between the apparatus and the microscopic system, or due to the coupling between the apparatus and the environment[24], becomes a question for experiment.

References

[1] E. Wigner, Z. Phys. **133** (1952), 101; Am. J. Phys. **31** (1963), 6.

[2] J. Jauch, E. Wigner and M. Yanase, Nuovo Cimento **48** (1967), 144.

[3] J. Bell, Helv. Phys. Acta **48** (1975) 447; Physics World **3** (1990), 33.

[4] C. Darwin and R. Fowler, Philos. Mag. **44** (1922), 450; 823; Proc. Cambridge Philos. Soc. **21** (1922), 391.

[5] A. Khintchine, *Mathematical Foundations of Statistical Mechanics*, Moscow, 1943.

[6] G. Ford, M. Kac and P. Mazur, J. Math. Phys. **6** (1965), 504.

[7] J. Lewis and H. Maassen, *Lecture Notes in Mathematics* **1055**, Berlin, 1984, 245.

[8] E. Farhi, J. Goldstone and S. Gutmann, Ann. Phys. (NY) **192** (1989), 368.

[9] H. Green, Nuovo Cimento **9** (1958), 880.

[10] A. Daneri, A. Loinger and G. Prosperi, Nucl. Phys. **33** (1962), 297.

[11] K. Hepp, Helv. Phys. Acta **45** (1972), 237.

[12] J. Schwinger, J. Math. Phys. **2** (1961), 407.

[13] W. Zurek, Phys. Rev. D **24** (1981), 1516; **26** (1982), 1862; Physica Scripta **76** (1998), 186.

[14] C. Gardiner and P. Zoller, *Quantum Noise*, Berlin, 2000, pp. 212-229.

[15] M. Collet, G. Milburn and D. Walls, Phys. Rev. D **32** (1985), 3208.

[16] J. von Neumann, *Mathematicsche Grundlagen der Quantenmechanik*, Berlin, 1932.

[17] P. Dirac, *Directions in Physics*, New York, 1978, p. 10.

[18] R. Feynman and A. Hibbs, *Quantum Mechanics and Path Integrals*, New York, 1965.

[19] A. Sudbery, *Quantum Mechanics and the Particles of Nature*, Cambridge, 1986, 41ff.

[20] J. von Plato, "Ergodic Theory and the Foundations of Probability," in *Causation, Chance, and Credence, Proceedings of the Irvine Conference on Probability and Causation*, edited by B. Skyrms and W. Harper, vol. 1, Kluwer, 1988, pp. 257-277.

[21] R. Minlos, *Introduction to Mathematical Statistical Physics*, Providence, 2000, p. 22.

[22] W. Pauli, "L'apport d'Einstein à la théorie quantique," in, Schilpp, ed., *Einstein*, Evanston, 1949.

[23] K. Hannabuss, Helv. Phys. Acta **57** (1984), 610; Ann. Phys. (NY) **239** (1995), 296.

[24] H. Zeh, in *Proceedings of the II International Wigner Symposium*, Goslar, Germany, 1991, edited by H. Doebner, W. Scherer, and F. Schroeck, Jr., World Scientific, 1993, 205.

[25] J. Johnson, Statistical Mechanics of Amplifying Apparatus, Paper presented at the VIII International Wigner Symposium, New York City, May 26-June 1, 2003.

[26] J. Johnson, in *Quantum Theory and Symmetries, Proceedings of the third International Symposium,* Cincinnati, 2003, edited by P. Argyres *et al.*, Singapore, 2004, 133.

[27] A. Allahverdyan, R. Balian, T. Nieuwenhuizen, Europhys.Lett. 61 (2003) 452-458

[28] J. Johnson, *Probability as a Multi-Scale Phenomenon*, Paper presented at the Special Session on Mathematical Challenges in Physical and Engineering Sciences, at the 1017th meeting of the American Mathematical Society, Durham, New Hampshire, April 22-23, 2006.

[28] J. Johnson, *The Logical Status of Probability Assertions*, submitted.